DEPARTMENT OF
HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Communicable Disease Center
Enteric Bacteriology Laboratories
P. O. Box 185
Chamblee, Georgia

December 15, 1955

Dr. J. Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

I doubt that anyone now at the Army Medical School could tell you anything about N97 (S. paratyphi B var. java or S. java, as Kauffmann calls it). Here is the story as I recall it:

About 1940 or 1941 the Army became quite concerned about what they considered a very extensive outbreak of gastroenteritis among troops in the Canal Zone. They forwarded a large group of cultures to N.I.H. who in turn sent them to us - hence, the preceding letter N on all the cultures. What they took for one outbreak was actually five different infections and the identity of the organisms corresponded with the geographical location of cases. One of these outbreaks was due to the java type. I do not recall how many cultures we had but I know there was quite a handful. Bailey Cherry worked with these cultures in 1944 or 1945, putting them in b and b + z_{33} serum. I believe he isolated 1,2 phases from N25 and N97 but from none of the others - 157 was derived from N25. You could refer to the culture as CDC 157 or as 157 (Edwards and Bruner, Ky. Agr. Exp. Sta. Cir. 51, 1941). The latter reference is the only actual published account. However, I think CDC 157 would be better since they long have been out of business in Lexington.

In regard to Kauffmann's phages, they were titrated in Maaløe's department and I think someone slipped up. I cannot imagine obtaining a double transduction (flagella and i antigen) with a phage having a titre of 10^7.

In re phage being the actual transducing agent - I was only being cautious.

I have more faith in the 5 transduction than you, probably because I have looked at thousands of cultures like S. abortus equi, S. derby, S. california, etc. without finding one with 5. Certain types apparently are devoid of 5 and S. abortus equi is one of them. Other types may or

may not have 5. I would not deny the possibility of 5 variation nor the fact that 5 may be a latent or covert antigen in types apparently devoid of 5. You may be wise in taking the stand you do.

I long have wanted to repeat some of the Japanese work on E_1 and E_2 but simply haven't the time. Some of that work looks very fishy to me - it was entirely too easily done.

I have read your enclosure and gone back over the pedigrees of the anomalous forms. Apparently the recipient usually determines the rate of phase variation in the "hybrid". Thus, it is perfectly logical that descendants of N97 should be very reluctant to vary in phase. Your discussion is started along the right lines, I believe. Before you are through with it you may convince yourself that you transdueed the gene for phase variation to S. abortus equi, as I have long contended.

It probably would be possible to devise a system to select for transfer of phase variation. Something like the following might do:

S. abony _____x N97 in b+ e,n,x sera
or
S. wien _____x N97 in b+ l,w sera

Since N25 and N97 throw 1,2 phases so rarely the emergence of a 1,2 phase probably would indicate transfer of the gene for variation. Systems also could be devised to use with CDC 157:

Typhi murium _____ x 157 in i + 1,2 sera

Do you think these might be worthwhile? I think it was something like this which happened in SW 1003 and similar cultures.

With kind regards, I am

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

Phil

Philip R. Edwards, Ph. D.
Bacteriologist-in-Charge
Enteric Bacteriology Unit

Best wishes to you and Esther for a happy Christmas. Our girls get home 12/17 and that will be fine. The house has been very empty with both of them away P.